

real or apparent, are succeeded by other periods of rapid transformation, during which what was previously only exceptional and abnormal becomes the regular state of matters. And, finally, we must not forget that to us time is only the succession of phenomena, and that, whether these phenomena appear to us to succeed one another slowly or precipitately, the result remains the same as regards the doctrine of evolution. In either case the principle of the continuity of things is in no degree affected.—*Comptes Rendus*, May 13, 1867, pp. 929–933.

The Theory of the Skeleton.

To the Editors of the Annals and Magazine of Natural History.

GENTLEMEN,—I do not imagine that readers of this Magazine will have forgotten Mr. Herbert Spencer's claim to date his views on the skeleton from 1858. I wrote to you not to dispute that, or to impugn Mr. Spencer's claim to be a great discoverer, but to vindicate my own claim to have honestly and independently thought out, from anatomical and physiological data, the theory of the skeleton which I had the honour to submit to your readers. I did not attempt to claim any credit, believing the pursuit of truth inconsistent with the pursuit of fame, and that fame is not honour when awarded at a man's measure of his deserts, but only when spontaneously conferred by his fellow thinkers. If the germ of the view published in my paper prove, as it may prove in its present or some other form, an addition to the philosophical groundwork of anatomy, Mr. Spencer may be sure that he will receive a full share of honour, if his claim is well founded; but till then, all haggling over priorities is waste of good time, which neither of us ought to be able or asked to spare from original work.

I have done myself the pleasure to read the review of Prof. Owen's theory of the skeleton, printed in the 'British and Foreign Medical and Chirurgical Review' (new ser. vol. xxii.), of which Mr. Spencer avows himself the author. And after much logical criticism, in which Prof. Owen's views are roughly handled, the review concludes with a page or two, much less logical, in which Mr. Spencer claims to have stated his discovery. So far as I can judge, the important passages in this statement are these:—

“The entire teaching of comparative osteology implies that differences in the conditions of the respective vertebræ necessitate differences in their structures.”

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“It is impossible to deny that if differences in the mechanical functions of the vertebræ involve differences in their forms, then community in their mechanical functions must involve community in their forms.”

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“..... have a community of function, it follows necessarily that they will have a certain general resemblance.”

In my judgment, this is only another and more emphatic way of stating the coordination of structure and function which has been insisted on by Prof. Owen and other naturalists again and again. In the first passage that I have quoted all this dependence of structure on "conditions" is assumed to be true. In the second passage, assuming it to be true, it is generalized into a law. In the third passage, assuming the existence of the law, its results are assumed to be tolerably uniform.

Now I am not aware that any number of assumptions, vague ideas, or guesses will make a discovery; and if they had done so, are we not entitled to assume that the discoverer, instead of publishing it anonymously, in a few vague sentences at the end of a review in a specially professional periodical, would have avowed his great thought, and brought it prominently before naturalists who could judge of its value? especially as he is now anxious to have credit for it.

I have also had an opportunity of referring to the 'Principles of Biology;' and although Mr. Spencer insists with admirable clearness on the correlation of structure and function, and, as in the review, on the modification of structures by "incident forces," I did not notice that these "incident forces" were defined; while, so far as I could understand, Mr. Spencer confessed that he did not altogether see how their results were produced.

If this is a correct statement of Mr. Spencer's vague hypothesis, I submit that, but for the terms "pressure and tension," and "mechanical theory," our views have little in common. His appears to me to have been an idea evolved out of an intellectual consciousness of what ought to be. My view was arrived at inductively from a long investigation; and it was only when I was assured by mathematicians, chemists, physicists, and others of their willingness to cooperate in eventually demonstrating the view, that I consented to publish a sketch of my method of studying the theory of the skeleton. For it is a part of a larger system referring the phenomena of nature to their ultimate and actual physical causes, many of which in their applications to life are discussed in a book of mine shortly to be published, on "The Dynamical Geology of Great Britain."

I am, Gentlemen,

Very faithfully yours,

HARRY SEELEY.

Note on the Phenomena of Muscular Contraction in the Vorticellæ.
By C. ROUGET.

Living muscles can alternately shorten and elongate themselves: this is their characteristic property. In purely elastic organs shortening only takes place after previous mechanical elongation; the muscles, on the contrary, can shorten themselves without appearing to have undergone any extension.

Whatever may be the causes of the elongation and shortening of the muscular fibres, whether these opposite states result from a